

Dear Arthur,

Dear Anna,
I am sorry not to have
replied more speedily to your letter
dated 7th October, but I have
been rather 'poetic', as I have been
lecturing both at Oxford and in London
and dressing backwoods, forward
and behind the scenes.
I have

Now that term has ended I have
settled back to read your interesting
paper on Carrolteas and Physical
This ~~years~~ ^{years} there were a lot of left out
~~and~~ ^{and} all very matter special
But first let me explain my point
about your 1977 proof that the Carrolteas
together with Casuarina and Fost., Carr
greenish toney

the unassisted locality can in, as I said, before you do not allow the poss.

that the value of $I \otimes B$ in the state $U(\phi \otimes \beta)$ depends on whether you are measuring A or $f(A)$ on the first spin. If you did not assume locality you would have to write

$$(1) [A \otimes I]_B^{U(\phi \otimes \beta)} = x_m \Rightarrow [I \otimes B]_A^{U_1}$$

and also

$$(2) [I \otimes B]_{f(A)}^{U(\phi \otimes \beta)} = y_m \Rightarrow [f(A) \otimes I]_B^{U(\phi \otimes \beta)} =$$

where you do notation $[I \otimes B]_A^{U(\phi \otimes \beta)}$ to indicate the value of $I \otimes B$ will reflect $U(\phi \otimes \beta)$ when the operator is set to measure A on the first particle, and

But then (1) and (2) if correct should be

$$[f(A) \otimes I]_B^{U(\phi \otimes \beta)} = f([A \otimes I]_B^{U(\phi \otimes \beta)})$$

Scarily because:

$$\{\mathbf{I} \otimes \mathbf{B}\}_{\mathbf{A}}^{U(\Phi^{\otimes 3})} = \mathbf{Y}_m \quad \text{A}$$

$$\{\mathbf{I} \otimes \mathbf{B}\}_{f(\mathbf{A})}^{U(\Phi^{\otimes 3})} = \mathbf{Y}_m$$

This implicates and goes through
if we don't distinguish $\{\mathbf{I} \otimes \mathbf{B}\}_{\mathbf{A}}^{U(\Phi^{\otimes 3})}$ from
 $\{\mathbf{I} \otimes \mathbf{B}\}_{f(\mathbf{A})}^{U(\Phi^{\otimes 3})}$ as in your published paper
and this is what I meant by saying you
assumed Locality.

I would then want to repeat the
your 1977 proof of inconsistency in
with a proof of Nonlocality, in
we decided to bring all to the framework
of Causation (which after all is a particular
case of the extended value rule for your
conceivable observables, when you allowed a
your 1974 Synthese paper.)

Do let me know what you think about this.

Now let me make a few comments on your new paper:

P-19. In your discussion of responsible indefinability, you note that its probabilities for each λ are captured by (P4) in that $p(ST, \lambda)$ is itself expressible in the form

$$p(ST, \lambda) = \int_0^1 S(x) T(x) dx \quad S(x, \lambda) T(x, \lambda)$$

In other words, in terms of a space of derived pairs $\langle x, \lambda \rangle$ with a product measure defined on it derived pair welfare measure and the P-measure

λ , we are writing

$$\begin{aligned} \phi \cdot \alpha(S, T) &= \int_{\Lambda} \phi(ST, \lambda) e(\lambda) d\lambda \\ &= \iint_{\Lambda} S(x, \lambda) \cdot T(x, \lambda) e(\lambda) dx \end{aligned}$$

So factorization has been worked at the $\langle x, \lambda \rangle$ level of description.

Now this is what I understand Shimony to be claiming. Not at a particular level of description factorizability holds and its failure for any given level of description is an indicator that the level is not open enough. Your

desire of operational indeterminism seems actually to bear out Shimony's claim, although I take it that your regard your disowning of a commitment to Shimony's support of CH in linking locality with factorizability is an genuinely Oxford and Cambridge affiliated factor in disavowal.

p.20 I am not happy with your disowning of Nelson's theorem. It is ambiguous what you mean by the remark 'each A_3 is more likely to correspond to some random variable A_3' . If this means A_3 is closer to that $\text{Prob}_{\text{S.H.}}[A_3]$ than $\text{Prob}_{\text{I.R.}}(A_3' | \Delta)$

for all B and \tilde{A} , where B is in the
notation $\{\tilde{A}\}_{A_i}^4$ for the projection of \tilde{A}
a value belonging to the set A in \mathbb{C}^n with

i.e. if you assume that A_i gives the
right probability distribution for A_i , according
to the statistical algorithm of $\mathcal{A}H$,
then it follows that

$$\langle \tilde{A}_i \rangle_{\mathcal{A}H} = \langle A_i \rangle_{n.v.}$$

$$\begin{aligned} \text{and } \langle \tilde{S} \rangle_{\mathcal{A}H} &= d_1 \langle \tilde{A}_1 \rangle_{\mathcal{A}H} + d_2 \langle \tilde{A}_2 \rangle_{\mathcal{A}H} - \\ &= d_1 \langle A_1 \rangle_{n.v.} + d_2 \langle A_2 \rangle_{n.v.} - \\ &= \langle d_1 A_1 + d_2 A_2 + \dots \rangle_{n.v.} \\ &= \langle S \rangle_{n.v.} \end{aligned}$$

But this makes your statement of Nelson's
theorem trivially false.
What Nelson did show was that if a
correspondence A_i is in the sense of his
assumption that there always exists a choice
of the coefficients d_i and set the probable
distributions for S and \tilde{S} that get ago
(although the expectation values will agree)

What Bell's argument does I would have thought, is that all random variables correspond to A^B , A^B' , etc. except for just $A(\lambda)B(\lambda)$, $A(\lambda)B'(\lambda)$, etc. then $A(\lambda)$ is a random variable corresponding to A and $B(\lambda)$ to B , etc. (again throughout my sense of the word correspondence of fact to see the connection here with Nelson's theorem: even if the correspondence is restricted to getting only the expectation values right. I fail to see the connection here with Nelson's theorem)

P.22 ff.
I believe the majority of your modern and present models: With regard to the former I feel the term conspiracy model might be more appropriate, if they could not be reproduced & of predictions in all circumstances and never allow the two 'possessed' systems

to be Nobel! With regard to the present
models I agree with this is possible
but feel that ~~do the doekhantok~~
and perhaps we must fall back to
doekhantok in quantum mechanics

It was a great pleasure to meet
you again last summer. Play of
with you and your family a happy
1981.

Yours ever.

Higgs